SCIENCE

NEW YORK, SEPTEMBER 1, 1893.

INDIAN RELICS IN SOUTH JERSEY.

BY JOHN GIFFORD, SWATHMORE COLLEGE, PA.

IT was the custom of the Indians to visit the seashore at certain times of the year. The trails they followed have been traced across the State of New Jersey. "Beach-day" and "clam-bakes" are customs learned from the Indians. The enormous quantities of shells in heaps along the shore are indications of these migrations and of their fondness for the oyster, clams and other mollusks. A certain kind of clam is still known by its Indian name, *quahog*. Many tons of these shells still remain in spite of the fact that large quantities have been used for roads, for farms and, long ago, for a flux in the manufacture of iron from bog-ore. The size and number of these heaps indicate that the bays and thoroughfares were then literally full of clams and oysters of considerably larger size than those of to-day. There is little else of interest in these heaps besides a few scattered potsherds.

Owing, perhaps, to the lack of fresh water, the inclemency of the weather and the noxious insects which infest these marshes, the seashore was but a transient resting place for the Indian. Tradition says that in spite of their endurance they were unwilling to bear, for any length of time, the bites of those pestiferous flies and mosquitoes.

From the physical geography of the region one may quickly judge where they would locate their permanent settlements. The sands of the interior offered few attractions. Water was their highway and the source of much of their food, so the majority of their villages were situated on prominent points of the rivers, not far from the bays and ocean, not far from fresh water, near fairly good soil, since he cultivated maize and perhaps pumpkins, beans and tobacco, near fresh-water "flats" where the "golden orontium" grows, the rootstalks of which were an important food, where he could find "snappers" or "logger-heads," as well as near a region of berries and game.

In many places in South Jersey the charcoal and grease of his kitchen-middens still blacken the ground. Here, too, are the bones of deer, turtle and other animals, bits of shells, pieces of Indian pipes, charred stones and other relics.

The largest rivers of that region are the Great and Little Egg Harbor or Mullica. On each of these there is the site of what must have been a very large permanent village. Vestiges are found in many other places in the neighborhood, but they are of little consequence in comparison with the region of Catawba on the Great Egg Harbor and of Chestnut Neck on the Mullica River. Two of the tributaries of the latter river are known as the Nescochaque and Mechesactauxin Branches. Another branch, called Edgepeling Creek, was the last resting place of the Indian in South Jersey and before their removal westward. With gratitude and frankness unlooked for in such barbarians, they credited the authorities with honest dealings toward their fathers and themselves.

The word Catawba, although an Indian name, is no way connected with the Indians who once lived there. It was named from the Catawba River, between the Carolinas, which received its name from the Catawbas who once lived along its banks.

Near Catawba, at South River, there are vestiges of an Indian village. Up the main river a short distance there is another at Goose Point. Throughout the whole region, in fact, there are signs of Indian habitations.

Catawba is a deserted sandbluff. Opposite are the fastnesses of a swamp forest. The river winds southward through many miles of marsh. So wild and deserted is the region that it requires but a little stretch of the imagination to see squaws picking berries along the banks or digging the rootstalks of the "Indian club;" others bringing clay from the beds near by, kneading and mixing it with bits of pounded quartz and sherds; others weaving moulds of grass and twigs; others ornamenting the finer grades with dots and lines; others working implements of jasper, and, perhaps, wampum, from shells. A group of wattled huts, thatched, perhaps, with the leaves of corn and calamus, surrounding a fire, on which there is a very large pot in which the rootstalks of the golden orontium are boiling, belongs to the picture.

Orontium aquaticum, so often spoken of by old writers as an important food plant, covers the flats of these rivers. It is believed by some to have been introduced by the Indians. It might be profitable to cultivate this plant, since it is not bad food, although it needs to be cooked full half a day to be palatable.

In the light and durable wood of the white cedar they found excellent and abundant material for their canoes. At Chestnut Neck, so called because of the chestnuts which once grew there, a canoe of chestnut wood was dug out of the marsh.

Chestnut Neck is much nearer the sea and is not so desolate as Catawba. The soil is richer, and the inhabitants well-to-do bay-men. Few Indian bones have been found in South Jersey in spite of careful searching. It may be that they carried the bones of their ancestors away, as did the Nanticokes.

Of all that they left behind them sherds are the most abundant, and fortunately, most valuable. Pottery is an unmistakable evidence of man. Natural formations simulate his handiwork, but pottery, no matter how coarse, is a sure sign of human habitation. It marks best the progress of culture, since that was one of the first, the most lasting and the easiest method of expressing his artistic fancies. The mud-pie was the germ of art. The cultus of a people is often too quickly judged by the coarse sherds which cover almost every campsite. They made common vessels for common purposes. With the distinction of vessels began the separation of artist and artisan. We must measure ability, therefore, by the finest specimens found. Thousands of these bits must be collected, and from these the finest must be selected.

No whole pots have been found, to my knowledge, in South Jersey, but from the curvature of the bits some of them were of very large size. Some of these sherds are not decorated at all, others show signs of more artistic ability than is usually accredited to the Indian. The majority are soft, coarse and mixed with bits of quartz and sherds. Some are hard and fine. Some contain holes

near the rim for a bail, indicating that they carried their vessels in their hands and not on their heads. They vary in color, owing to the nature of the clay. Some have peculiarly ruffled surfaces, due to the kinds of moulds in which they were formed. The majority were moulded in baskets of grass.

Some are ornamented with straight lines and dots, others with curved lines, and dots in curves. The simplest decoration is where the edge is dented, as does a baker his pies. Lines often cross each other to form square and diamond figures. The top is often fringed with highly decorative bands. Many of the markings similate the tracks of animals, and on a potsherd found by me at Goose Point there is a picture of a human hand beside another hand, as though in the act of gesturing. Some of these are covered with what a potter would no doubt call a "slip;" that is, a very fine clay mixed to the consistency of cream and smeared over the surface of the vessel.

The pots varied much in size but little in shape. They were mostly almost round, although the writer has found a few angular sherds. Clay pipes are often picked up in their kitchen-middens. These are rude and unornamented. This is worthy of special mention since this peaceful, diplomatic and friendly emblem was usually much ornamented.

Almost as common as sherds are the little slivers and pieces of flint. The jasper which they used was supposed to have been quarried by the Indians in Pennsylvania and was broken by pouring water on the heated stone, as obsidian is quarried to-day. It is interesting to note that the Indian of South Jersey found his jasper elsewhere in another form. This is indicated by the fact that the writer has found many pebbles of this stone partly chipped. On one of these there was the imprint of a fossil shell, which may be a clue to its origin. Arrow-heads, spear-points and awls of jasper have been also found. The slivers of this stone which are so common in spots were probably not chipped but pressed off by some sort of a revolving apparatus. This is indicated by the little round pits which may often be seen in unfinished flints.

Indian axes are very scarce. They were made of a stone which is not found in South Jersey, and owing to their weight sink quickly and are lost in the sand. Potsherds, fortunately, come to the surface.

Such are the faint vestiges of a people who by disease, gunpowder and deceit have been practically exterminated. Some day archæologists will study the pieces of crockery, glass and brickbats, wonder over the old tin cans and brass heads of gun-shells which we leave behind us, and perhaps pronounce our cultus high in the arts and sciences; but in selfishness, the commonest human quality, we are, perhaps, but little, if at all, the Indian's superior.

NOTES ON SOME MINNESOTA MOUNDS.

BY ALBERT SCHNEIDER, UNIVERSITY OF ILLINOIS, CHAMPAIGN, ILL.

In the summer of 1892, while engaged on the zoölogical survey field work of Minnesota, I happened across a considerable number of "Indian Mounds." They were especially common in the Mille Lacs Lake region. All those observed were situated within a few rods of the old shores of Mille Lacs Lake, or of some of the numerous smaller lakes near it. They were all of about the same size and appearance, 40 to 50 feet in diameter at the base and 4½ or 5 feet high. As to the age of these mounds nothing definite can be stated; they are evidently of comparatively recent origin. Some had trees growing on them 2½ feet in diameter. It is reasonable to suppose that they are from 250 to 500 years old.

At Lake Warren, a small lake near the outlet of Mille Lacs, I dug into one of these mounds. Acting under the impression that they were burial mounds, I located a central point and dug perpendicularly downward. At a depth of about 5 feet I reached the level of the surrounding soil. Nothing was noticed but some ashes and fragments of charcoal, indicating that a fire had been kindled on the grave before the mound was built. Continuing the excavations I found the opening of the grave, which was about 4½ feet long by 3 feet wide, and gradually tapering downward to a rounded bottom at a depth of 4 feet. The hole was evidently dug with some crude instrument, as the roughness of the sides would indicate. In this one grave were found the bones of four human bodies and the scales of some fish. The bodies were arranged side by side in a sitting posture, with the legs and arms strongly flexed upon the body and the back toward the side of the grave. From the examination of the bones I made out the following points: One was a child of about six years, another that of a young person of sixteen or seventeen years, the third that of a middle-aged, medium-sized woman, the fourth that of a short, heavy-set, muscular man about fifty years of age. This man's teeth were very much worn, though none were decayed. In fact, all the teeth found were in good condition. Some of the vertebræ, the leg, arm and hip bones were well preserved. Only a few bones of the child were found and it was difficult to determine its position in the grave. It was apparently placed in a sitting position in the woman's lap. No utensils or implements of any kind were found. The sandy soil which made up the mound and filling of the grave was taken from a spot some ten rods distant, leaving a shallow depression.

Numerous pieces of pottery have been found in this region, mostly plain, some with crude ornamental markings near the rim. All pots or vases were rounded. Stone implements were also found. Copper implements were reported to have been found, though I was unable to see them.

The most interesting feature of the grave described is that it contained four bodies, apparently an entire family. How came they to be in one grave and evidently placed there at the same time? The probable supposition is that some epidemic carried away large numbers. In that case would it be likely that the survivors would build mounds over all graves? Or were only those of distinction honored with burial mounds? It is necessary that more mounds be studied before these questions can be answered. No scientific examinations have as yet been made of the Minnesota mounds.

It is probable that there is a close connection as to the time of formation of the "Indian Mounds" of Illinois and Minnesota and the noted "Animal Mounds" of Wisconsin and other states.

In closing, I wish to call attention to the necessity of thoroughly and systematically studying these mounds within the next few years, else the farmer and amateur archæologist will make useless and destroy all.

A UNIVERSITY course of thirty lectures on "Celestial Mechanics" by G. W. Hill, member of the National Academy of Sciences, Honorary Doctor of Sciences of the University of Cambridge, England, will be given in the astronomical lecture room, Hamilton Hall, Room No. 28, Columbia College, on Saturdays at 10.30 A. M. The course will begin on Oct. 14, and continue every Saturday until finished, omitting Saturdays, Dec. 23 and 30. The lectures are open to the public without fee. The course will be confined to the motions of the heavenly bodies considered as material points. Dr. Hill will give a somewhat full presentation of the subject rather than a rapid *resume.* Short numerical illustrations will enable the hearer to comprehend the bearing of the principles enunciated on practical work.

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CURRENT NOTES ON CHEMISTRY.—II.

[*Edited by Charles Platt, Ph. D., F. C. S.*]

FLASH POINT OF MINERAL OILS.

WHILE not strictly chemical in its nature there are but few scientific tests so intimately connected with our safety as the determination of the flash and burning points of mineral oils. This has long been a matter of concern to oil merchants alone, but the scientific public is now taking an interest in the matter, which it is hoped will decrease, if not do away with altogether, the vast number of preventable lamp explosions and fatalities.

The safety of an oil is determined by its flash point, that temperature at which an explosion occurs when a flame is applied to the mixture of air and vapor immediately above the surface of the oil. A flash occurs, but the oil does not take fire and burn continuously, in the ordinary test cup, until a higher temperature is reached, its *burning* or *firing* point. Originally the test was applied to the oil in an open cup, but, this method introducing many chances of error, a closed cup was finally adopted, the flame being inserted through a hole in the cover. 100° F., formerly considered as the minimum safety point for oil, in the open cup corresponds to 73° F. in the closed test, and with the adoption of the latter, the British Government, advised by Sir Frederick Abel, lowered the minimum safety point required by law to this temperature! The reports and papers by Sir Frederick Abel and by Mr. Redwood, who was associated with him, contain many outrageous assertions, among others that an oil flashing at a low temperature is more safe than one flashing at a high temperature. They argued that by using the low-test oils a greater volume of vapor is given off and the air is thus driven from the lamp. A metal lamp was also recommended as the safest on this same principle, that by the heating of the oil in the lamp reservoir vapors are evolved from the oil, and the air being driven out as before, an inflammable, but not an explosive, mixture is obtained. When we consider that 73° F., adopted by the British Government, is a temperature frequently exceeded in our houses, the danger of such a ruling is apparent. Mr. D. R. Steuart presented an admirable paper to the Glasgow and Scottish Section of the Society of Chemical Industry, early last winter, in which the fallacies of Abel's position were forcibly shown. His paper was thoroughly discussed by the members at that meeting and subsequently, with the final result of an appointment of a committee of experts to pass upon the question. Their report fully sustained Mr. Steuart and recommended a higher flash point of minimum safety than that now established by law. Mr. Steuart's paper, presented at that time, and others of more recent date, contain many interesting facts relative to the burning of oils, as, for instance, the relation between flash point and heat developed in burning, the effect of the presence of heavy oils, of chemicals, etc., and of the size of the container.

A lamp burning badly develops more heat than usual, the light is red and the combustion imperfect, producing a disagreeable odor. This may arise from the air not being properly reverberated against the flame; or from the shape of the chimney allowing of back currents; or from the lamp being dirty, the air holes clogged, the wick damp or dirty; the presence of a trace of vegetable or animal oil in the vessels used for filling; or from the oil itself, the presence of heavy oils or refining chemicals. When the oils are not homogeneous, a light and heavy oil being mixed, the heat developed is greater than with either oil separately, this result being more pronounced when a poor wick is used. A well fractionated oil is practically independent of the wick. The treatment of the oil after the last distillation with acid and alkali, results in injury to it, no matter how thorough the final washing. Sulpho compounds of soda are often retained, and these decompose in the burner, forming sulphuric acid, which chars the wick. Carefully fractionated oils are low or high, in flash, in proportion to the specific gravity and boiling point. A low-flashing oil gives the highest temperature in burning. (Contrary to Abel and Redwood).

Another feature has been brought to our attention lately, that of the influence of the size of the containing vessel upon the danger point in oils. The Abel test, it will be remembered, is prescribed as a two-inch cup. A particular sample flashed in Abel test at 78° F.; in the old government open test at 105° F., and *fired* in the old government open test at 122° F. Although a small cup of this oil cannot supply vapor sufficient for a constant flame below 122° F., a larger surface can. The oil above mentioned, tested in an apparatus like the old government open, with a screen around and partly on top, but nine inches in diameter, applying the flame every two degrees, ignited explosively at 88° F. and continued to burn furiously. Applying the flame every degree the same result was attained at 87° F. Transforming the apparatus into a closed test, the oil ignited and burned at 76° F. Except, then, for small surfaces, the flash and burning points are the same, and the Abel flash, becomes a point of danger for oil in store, barrel or tin, while for oil in large vessels, tanks, etc., the danger point is still lower. A case is cited where a large tank of very high flashing oil was being pumped into, the temperature being far below the flash point in Abel cup, vapors were evolved, overflowing through an imperfectly closed manhole at the top, and were ignited at a lamp some distance below. The fire ran back; an explosion resulted, blowing off the top of the tank, and the oil was burned. It is curious to note that while the British Government fixes the flash test at 73° F. for the public, it places the same at 105° F. for its own governmental departments, and at 145° F. for the lighthouses.

EXTRACTION OF FAT FROM FEEDING CAKES.

The extraction of fat from fodder by means of anhydrous ether, after a preliminary drying, or even with low-boiling petroleum, is known to be unsatisfactory. To avoid the simultaneous extraction of coloring matters, resins, waxy impurities, etc., Dr. L. Gebek has conducted experiments, using burnt gypsum mixed with the substance to be extracted, also filtering the ethereal solution through a gypsum filter. Finely powdered gypsum be-

coming impervious during use, a granular material was obtained by powdering plaster figures, igniting and passing through a 2 mm. sieve. The substance was air-dried and ordinary ether used. Anhydrous ether apparently did not affect the results, though these were lowered by a previous drying of the food stuff. The extracts, though pure, were not constant in weight. Spanish earth was found to yield satisfactory results after the following procedure. The fine powder was mixed with water, sufficient sulphuric acid added to remove the carbonates, and the whole evaporated to dryness and ignited. The mineral was then powdered and passed through a 2 mm. sieve. A cotton plug is inserted in the end of the extraction tube, and upon this a layer of 3-4 cm. of Spanish earth, after which a mixture of the earth and fodder and then another plug. 12-15 grammes of the earth were used for 5 grammes of the fodder. With ordinary fodders the results were the same whether hydrous or anhydrous ether was employed, but with foods rich in fat lower results by a few tenths were obtained with the anhydrous. A previous drying of the substance, when Spanish earth is used, gives low results, probably due to the retention of that portion of the fat which may have been changed by the action of the heat.

SYNTHESIS OF PURPUREO-AND LUTEO-CHROMIUM CHLORIDES.

Professor Christensen, of Copenhagen, has produced by direct synthesis the so-called purpureo-and luteo-chromium chlorides, $Cr\,Cl_3\ 5\,NH_3$ and $Cr\,Cl_3\ 6\,NH_3$. A small quantity of violet chromium chloride, dried at 100°, *is placed in a beaker and immersed in a freezing mixture of solid carbon dioxide and ether. Liquid ammonia (NH_3) is slowly added. At this temperature no reaction takes place, but upon removing from the freezing mixture and warming to —38.5°, the boiling point of ammonia, a sudden reaction sets in, converting the chloride into a red mass, consisting largely of the purpureo-chloride. The excess of NH_3 is eliminated as gas. The product is washed with cold water and hydrochloric acid, finally dissolved in water and the solution dropped into concentrated hydrochloric, in which the purpureo-chloride is insoluble, when the red crystals of the pure salt are thrown down. The first aqueous washings are yellow and yield a yellow crystalline precipitate of luteo-nitrate upon the addition of concentrated nitric acid. The reaction takes place between very narrow limits—immediately above and below the boiling point of ammonia —38.5°.

DETERMINATION OF GERMANIUM.

Quantitative estimations of the rare metals being unknown to text-books on chemistry, the methods adopted by experienced analysts have a decided instructive value. The following is the procedure in an analysis of the new mineral canfieldite as given by Mr. S. L. Penfield in the *Am. Jour. of Science*. A preliminary qualitative examination was made showing the mineral to be essentially a sulpho salt of germanium and silver. The silver and sulphur were determined as usual. For the germanium, 2 grammes are oxidized with nitric acid, a little sulphuric being added and the excess of nitric removed by evaporation to dryness. The residue is dissolved in water, which has been rendered slightly acid, if necessary, and the silver precipitated with ammonium thiocyanate, filtered and the filtrate containing the germanium collected. The solution is evaporated to dryness in a platinum dish without danger, no acid being present to form with the germanium a volatile compound. The excess of sulphuric acid is driven off by heat, and the ammonium thiocyanate is destroyed by the nitric acid present. The residue is covered with a little strong ammonia (NH_4OH) into which sulphuretted hydrogen is conducted, thus dissolving the germanium oxide and leaving all heavy metals, except those which form sulpho salts soluble in ammonium sulphide, undissolved. The filtrate from this solution is collected in a platinum crucible and evaporated on a water bath, the residue oxidized by concentrated nitric, and the excess of the latter removed by a second evaporation. The mass in the crucible is now gently ignited and weighed, the germanium being determined as the oxide, GeO_2. There is no loss of weight on subsequent heating to a red heat.

Another scheme by which all of the determinations are made in one sample is briefly as follows: Solution in nitric; precipitation of the silver by means of hydrochloric; precipitation of the sulphur with barium nitrate; removal of the excess of chlorine and barium, in one operation, with silver nitrate and sulphuric acid; removal of the silver by means of ammonium thiocyanate; and the final determination of the germanium as above.

*Throughout these articles temperatures will be given in Centigrade unless otherwise stated.

THE WORLD'S CONGRESS AUXILIARY OF THE COLUMBIAN EXPOSITION.

BY GEO. H. JOHNSON, SC. D., ST. LOUIS, MO.

ONE of the greatest attractions of the Columbian Exposition is outside of the exposition. In the World's Congresses we have an exhibit of the world's intellectual progress and present condition such as has never been attempted before. For the first systematic attempt to make such a comprehensive exhibit of the world's thought by spoken language only the congresses have been very successful. During the whole of the six months that the fair is open the Memorial Art Palace, foot of Adams Street, Chicago, is the place of assembly for those who are prominent in any branch of theoretical and practical learning. At the fair we see the magnificent work of great masters. At the Art Palace we see the great masters themselves. As the creator is greater than his work, as thought is greater than action, so are the world's congresses greater than the fair.

It has been said that President Bonney, since the first day of May, has done nothing but open congresses; and indeed, that is quite sufficient to keep him busy, since several congresses meet each week, and each one is opened by Mr. Bonney with felicitous remarks appropriate to the subject.

Little effort, apparently, has been made here to show the intimate relations which exist between different departments of science and art. To attend one congress and then another exhibits as complete a change as to pass from Machinery Hall to the Fine Arts Building. Since the congresses are designedly meetings for specialists, it is to be expected that very few can take a prominent part in more than one congress. But the wisdom of such a complete separation between dependent and cognate subjects as some of the programs show, is open to question. For example, the Congress on Higher Education did not consider University Extension because the latter subject was considered exclusively in its own congress. The engineering educators could not attend any of the meetings of the civil, mechanical, naval, mining, metallurgical, or military engineers without leaving their own meeting, since all these and others were in session simultaneously.

Perhaps the greatest need of coöperation between closely related specialists was shown in the congresses on experimental and rational psychology. These meetings were held simultaneously in opposite halls of the institute, and each succeeded remarkably well in ignoring the work of their opposite brethren. Indeed, it might have been inferred from some of the remarks that what is experimental is not rational, and what is rational will not bear the test of experiment. A professor in one famous

university, in summing up his criticism on experimental psychology, said that the new results of that science, for example, Weber's law, were not strictly true; and their true and valuable results had been set forth centuries before in rational psychology. In the other congress, shortly after, I heard the representative of another great university say that a single study in experimental psychology, carefully worked out, was of more value than all the works on rational psychology which had ever been written. A friendly rivalry between the advocates of different methods is probably stimulating and favorable to the development of science; but the depreciating of all methods except one's own, and the rejection or neglect of results obtained by other methods, is certainly detrimental to the specialist himself, and it lessens the reliability of his work. A conference between all those interested in psychology would have been very desirable.

There were some surprises at these congresses for which the programs could not prepare us. At the Congress on Rational Psychology, over which the venerable ex-President McCosh presided, some *irrational* speakers persisted in making themselves prominent when the subjects were open for discussion. On the other hand, at the Conference on Aerial Navigation, where some people went expecting to see the "cranks," there was nothing but plain statements of observations made, experiments tried, results achieved and theorems proved. At no other congress, perhaps, was there such a pressure of really valuable and original matter. The three days set apart for the conference, with doubt as to whether so much time would be needed for the discussion of such an embryonic art, proved to be quite insufficient; and even a fourth day did not give time for the reading of several valuable memoirs offered by practical and scientific men who are devoting much of their time to arts aerial without hope of any immediate financial return.

The Congress on Woman Suffrage was notable for the large number of men present who seemed to enthusiastically support the claims of their sisters. The Congress on Jurisprudence and Law Reform, where the most serious debates might have been expected, was characterized by the amusing stories and reminiscences of venerable judges.

The Congress on Social Settlements was a very earnest conference between ardent young college graduates, who constitute most of these settlements, and philanthropists and socialists.

The number of eminent visitors from abroad who have participated in most of these congresses has been sufficient to make the term "International" no misnomer. So many valuable papers have been read at these meetings, and the average excellence has been so high, that it is very desirable that the proceedings of all the congresses, including the discussion of papers, should be published in uniform style, fully indexed, and offered for sale at a price to secure a large circulation. An effort is to be made to have such an edition published and widely distributed by our government. The whole work would be a kind of thesaurus of practical knowledge. The theorists and visionaries have contributed their part to each subject, but generally it has been only a subordinate part; and the proceedings as a whole have been characterized by great practical wisdom.

The World's Congresses have been a kind of university for which the fair has served as museums, laboratories and recreation grounds. The congresses, although they have the mottoes, "Not things, but men," "Not matter, but mind," are officially designated as "auxiliary" to the exposition; I am inclined, however, to consider the exposition as auxiliary to the congresses.

A NEW FACTOR IN FRUIT GROWING.

BY B. T. GALLOWAY, WASHINGTON, D. C.

During the past three years the Division of Vegetable Pathology in the U. S. Department of Agriculture has been engaged in the study of twig or fire blight of the pear and apple. In the course of these investigations, which were for the most part carried on by Mr. M. B. Waite, an assistant in the Division, an attempt was made to obtain some definite information in regard to the relation of insects to the disease in question. As a result of this work it was shown that the organism causing blight was disseminated by insects during their visits to the blossoms. The blossoms, it was found, were readily infected by the pear blight germs brought to them by insects, the result being the death of the flower and frequently the twig or branch supporting the latter. This discovery raised the question of the necessity of insect visits to the flowers of pears and other fruits affected by blight. It was thought that if by some practical means insects could be excluded from the flowers without interfering with the fruitfulness of the trees, one form of blight at least might be prevented.

In order to obtain some information in regard to the effect on fruitfulness of excluding insects a series of experiments were made at Brockport, New York, in the spring of 1891. The results of these trials were somewhat startling, as it seemed to indicate a fact hitherto overlooked by scientific and practical men, viz., that many of our well-known varieties of pears will not set fruit unless their flowers receive pollen from other varieties. In other woods, the visits of insects, by means of which cross-fertilization is effected, is necessary to insure proper setting of the fruit.

To obtain further information on this subject more extended experiments were made on this subject in 1892 and 1893. This work was carried on in Virginia, New York, and New Jersey, the results in every case confirming those obtained in 1891. The facts obtained by these investigations seemed sufficient to warrant the important conclusion that most of our common varieties of pears and apples are unable to fertilize themselves. This law can hardly be called new, for Knight, Darwin and others have touched the same point in a broader and more general way. Strange to say, however, no one, up to the present time, seems to have applied the conceptions of Darwin and others on this subject to some of our common fruits, although it has long been recognized that orchards of pears, apples, plums, etc., fail to bear fruit regularly, even under the most favorable conditions.

In the light of our present knowledge it is known that unfruitfulness, in many cases, is due to the fact that large blocks of single varieties have been planted. In such cases there is not sufficient foreign pollen to effect fertilization, consequently the trees bloom profusely but no fruit sets. The new factor, therefore, which confronts the grower of pears and apples is to select his varieties and plant them in such a way as to insure cross-fertilization. Of course, in doing this it will be necessary to observe a number of important points, the details of which need not be given here. Suffice it to say that the time of flowering of the various varieties must be kept in mind in selecting those designed for pollinating. Then again, the question of the potency of the pollen with respect to the variety it is intended to grow must of necessity be considered, and, finally, it will be important to know what proportion of pollinating trees to trees it is desired to fruit should be planted.

RAILROAD SIGNALING.

BY REGINALD GORDON, COLUMBIA COLLEGE, NEW YORK.

THE chief object of signaling on railroads is to inform enginemen positively, at given points, whether they must stop or proceed, and the universal method of conveying this information is by a visible signal. Audible signals have been tried, but their use is rather limited. At the high speeds now usual on our railroads, an engineer ought to be able to interpret the meaning of a signal at some distance before he reaches it, so that if obliged to stop, he can bring his train under control and stop it, before reaching the point where actual danger exists, whether it be a train, an open switch, or some obstruction. The necessity of an easily distinguishable signal is thus obvious. In the early days of railroading, when trains were comparatively few and speeds were low, it was sufficient to have flagmen or watchmen, waving a flag by day, and a lantern at night. These men were of course governed by orders of the local superintendent or roadmaster. If a train stopped unexpectedly, or longer than usual, a brakeman was sent back with a flag or lantern to protect his train against a following one. Two great improvements were made in railroad operation when fixed signals, on poles or posts alongside the track, were adopted, and when the method of keeping trains apart, and protecting them, one against another, known as the "block signal" system was introduced. These fixed signals were moved by a combination of rods, wires and levers, worked by an operator situated at some distance and controlling, at the same time, several other signals in a similar manner.

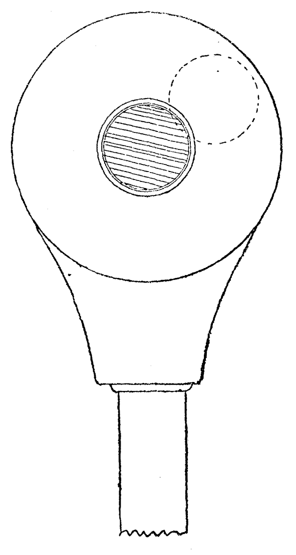

FIG. 1.

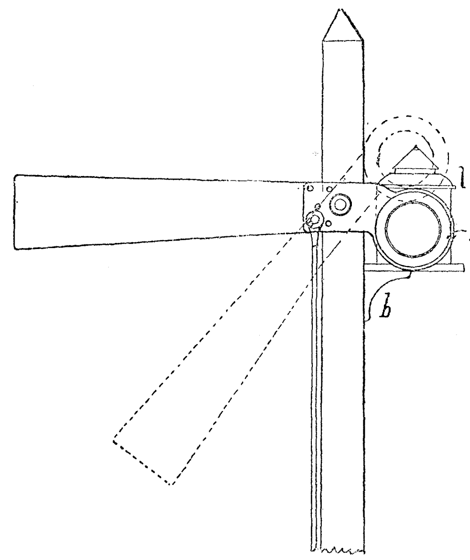

FIG. 2.

Of all the different forms for signals that have been tried, the disc and the semaphore are the only ones in use now. The disc signal may give its indications for safety, either by turning through 90°, so as to show only its edge, or by moving bodily out of that part of the signal case which, when occupied, means "danger." Fig. 1 shows an electrically operated disc signal at "danger;" and the dotted lines show the "safety" position. The semaphore signal, fig. 2, is more positive in its indication, because it is more easily discernible than a disc. The safety position of the semaphore is shown by the dotted lines in fig. 2. The blade or arm carries a frame, *F*, in which a red glass is fixed, so that at night, when the blade is raised to indicate danger, the lamp, *l*, fastened on the bracket, *b*, will show a red light. When lowered for safety, the lamp is uncovered, and, of course, shows white. The introduction and recent rapid extension of block-signal systems of various kinds has led to the almost universal employment of semaphores, and even where an absolute block system is not maintained, train movements, either on the open road, or within yard limits, are controlled mainly by this form of signal. The necessity of keeping two trains apart by a space interval, rather than by a time-interval, has been demonstrated in a most forcible manner by the long list of rear-collisions on railroads relying solely on the rear brakemen to keep trains apart. In the former method the road is divided up into spaces or blocks, and no train is allowed to enter any block unless the last preceding train shall have passed beyond its limits. The limits of each space or block are marked by signals, usually semaphores, operated directly by a signalman, or else controlled by him through the intervention of compressed air and electricity. The safety of trains, then, rests mainly upon the faithfulness of the signalmen, as well as the vigilance of the locomotive engineers.

Under the time-interval system, trains are not allowed to follow one another closer than after an interval of five, seven or ten minutes, according to the class of trains and their relative speed. With this system everything depends upon the faithful and active performance of duty by the rear brakeman of a train. Some railroads, unable to incur the expense of installing and maintaining a first-class block-signal system, have provided signals at every regular station, where the station agents, being in telegraphic communication with one another, can, if necessary, carry out a very fair "absolute" block system. Each station, then, marks the end of one section and the beginning of another. In times of heavy traffic, however, these blocks between stations are too long; that is, trains are kept too far apart, and compelled to wait so long at stations that the road could not be kept clear, and the service would become demoralized if this method were strictly adhered to. For these reasons, and under the circumstances cited, it is quite usual to allow trains to follow one another after an interval of time, determined in each case to suit the circumstances, and the practice thereby becomes "permissive," as opposed to "absolute" blocking. This method of operating is in use at present on many roads, and, though it no doubt prevents many collisions, is vastly inferior to an absolute, interlocking system of block signals.

Railroads with very heavy traffic, and traversing thickly settled regions, have lately found it necessary and expedient to equip their lines with this latter system, and it

is a question of only a few years before all our great trunk lines, or, in fact, all lines running trains at high speed will be thus protected. The earlier forms of block systems comprised a semaphore for each track, controlled from a cabin or tower at the entrance to each block or section. Telegraphic communication was established between these towers, and the movement of trains thus pretty well controlled, of course always assuming proper vigilance and devotion to duty on the part of the tower men and engineers. Nevertheless, accidents have happened by reason of a signalman forgetting that a train has lately passed his tower, and allowing another to fol-

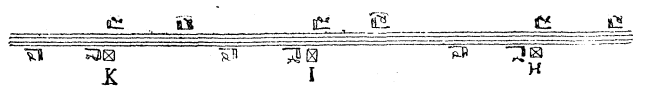

FIG. 3.

low it, without any information from the tower ahead. In the latest systems brought into use, the danger of such carelessness is largely, if not entirely, overcome, by interlocking the signal levers in two successive towers. By a combination of mechanical and electrical devices, each lever that moves a signal is locked in position by the man in the tower at the farther end of the block section, and can be unlocked only with the latter's consent and coöperation. For example, in fig. 3, a signalman at *H* cannot lower his signal to "safety," in order to admit a train to the block ahead, without asking the operator at the next tower, *I*, to unlock his (*H's*) lever. The man at *I* will not do this unless he knows that the block or section *H-I* is clear. A train having passed *I*, going towards *K*, and protected by a danger signal at *I*, the signalman there, on request of *H*, will unlock the latter's signal

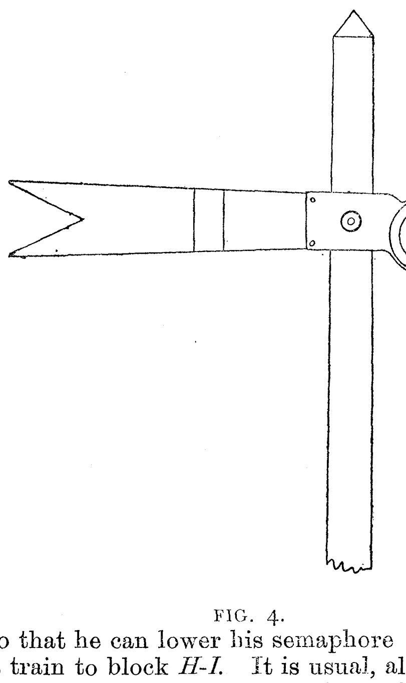

FIG. 4.

lever, so that he can lower his semaphore to safety, and admit a train to block *H-I*. It is usual, also, to have each signal in duplicate; that is, a semaphore placed from 1,200 to 1,600 feet in advance of the one at which an engineer must stop, if it stand at danger. The latter is called the "home" signal; the former, the "distant" signal. Home signals are almost invariably painted red, and of the form shown in fig. 1. At night they display a red light when the blade is raised to danger position. Distant signals are made of the "fish-tail" form, as shown in fig. 4, and painted green or, rarely, yellow, displaying a green light at night when raised to indicate "caution." A distant signal is for the purpose of informing an engineer whether he will find the home signal at danger or not. In moving the blades to indicate danger, the distant is first raised, then the home signal. In lowering them, however, the reverse order is used. If an engineer finds the distant signal lowered for him, he can go on confidently without slackening speed, knowing that he has a clear block ahead. If, however, it is against him, he then has time to bring the train under control and come to a dead stop on reaching the home signal, which, if at danger still, he must under no circumstances pass. In the Fourth Avenue tunnel, New York City, the signals are arranged so that the act of moving a signal to danger, places a torpedo on the rail over which the train must pass, and in addition to this, a gong is set loudly ringing if an engineer, neglecting the ordinary signal, runs beyond a certain point. Setting the signal to safety again removes the torpedo and throws off the gong mechanism. These extra safeguards have been found to be absolutely necessary in this place, where the traffic is so dense and the conditions of working are so trying.

ALTITUDE IN SPITE OF HUMIDITY AS A CURE OF BERI-BERI.*

BY ALBERT S. ASHMEAD, M. D., NEW YORK.

THE Hakoné Mountain resorts, 836 metres above sea-level, Karuizawa, the new foreign resort, the religious stations (ten) disposed on each of the four roads up the sacred Fuji Mountain, and the Ikao Mountain and hot springs resort at Nikko, are the main beri-beri resorts of Japan. All these are in the neighborhood of volcanic centres. Karuizawa, at the head of the Usui Pass, is 3,000 feet above sea-level. Its mean temperature is 8° lower than that of Tokio, in the principal Kakké month, August; and there is a mean oscillation of 20° F. in the temperature of the day, as compared with the night. While at Tokio the variation is only 14°. It is this coolness of the nights, in all the mountain resorts of Japan, which makes the heat of the day tolerable. The August humidity, in all the mountains of Japan, although they have three times the rainfall of Tokio, is practically the same as in the latter city.

Yamanaka, another resort in the Hakoné Mountains, is higher even than Karuizawa, the same conditions as above.

Fuji, the peerless mountain of Japan, is 12,238 feet high. Its slopes are cultivated to an elevation of 2,000 feet. It can only be visited in the Kakké season, July and August. At other seasons, it is too cold. The highest temperature that has ever been recorded in August, on the summit of Fuji, was 70.5°, and the lowest 31.1°. The mean daily range of temperature is a little higher, 20.9°, than at Karuizawa; that is the variation between day and night. There are at the top of the mountain thirty-six inches of rainfall, and three-fourths of the whole quantity belong to the three or four days of the first storm of the month. The influence of Fuji in encouraging precipitation, is shown also at Karuizawa, the latest beri-beri resort, and in the other resorts.[1]

The comparison between the three, top of Fuji, Yamanaka on the Hakoné Mountains, and Karuizawa, gives the following figures:

	Bar.	Range.	Temp.	Range.	Vap.	Hum.	Rain-fall.	Rainy days.
Top of Fuji.........	490.7	13.1	7.7	11.6	5.5	71.2	888.1	18
Yamanaka (Hakone).	677.5	11.8	20.6	9.6	16.0	88.7	580.4	18
Karuizawa.........	679.13	10.	21.3	11.1	16.0	86.	212.0	17

*Communicated to the Sei-I-Kwai, or Society for the Advancement of Medical Science in Japan.

1Ashino-yu is at Ubago, near the base of Fuji. Hakone Lake is separated from Fuji by a ridge. Yamanaka Lake is seen from the top of Fuji. The same influence operates at Ikao; this is near Asama-yama, the second highest volcanic peak in Japan (Shinano.)

The meteorological station which is nearest these resorts is called Numadzu; it is at sea-level, about twenty miles west of the Hakoné Mountains, on Sugura Bay, Pacific side of Japan. The notations for August are:

Bar.	Range.	Temp.	Range.	Vap.	Hum.	Rainfall.	Rainy days
757.6	12.7	25.8	7.2	20.5	83,	187.2	23.

The average humidity at Tokio, sea-level, for the three Kakké months, June, July and August, as given in the meteorological summary, is 81.6. This figure is inferior to that shown by mountain resorts.[2]

It will be seen that recovery from beri-beri takes place in these noted places, in spite of their excess of humidity and rainfall, which makes it evident that the humidity and rainfall of the Kakké months, June, July and August, at sea-level, in the beri-beri centres, cannot be a direct cause of the outbreak.

Another cause must be looked for. The history of the following case shows that a high altitude is absolutely necessary for a real cure of Kakké.

A patient of mine, M. H., 23 years old, a ship builder and a powerful man, by no means anœmic, a native of Kochi (a city of 50,000 inhabitants, not a beri-beri centre, on the sea-level, island of Shikoku), contracted beri-beri in Tokio, June, 1885. He was a patient at that time of Dr. Ikeda, the emperor's physician. He was ordered to the mountain, but his father insisted on his returning home. He recovered and came back to Tokio in October. In June, 1886, the disease reappears; this attack is stronger than the last; for ten days he is unable to walk at all. He has this time the attendance of Dr. Sasaki. That eminent physician tells him that he must stay in the mountains near Tokio, and not return home, if he wants to be cured for good and all; should he go back, thinks the doctor, the cure would only be temporary. The patient disregards this advice, and goes again to Kochi, and recovers in September, as before. He arrives again in Tokio in November, spends there the winter and the following spring. In May, 1887, there comes upon him a third attack, not a strong one this time. As usual, he retreats to his native place, and recovers in August. He betakes himself to Yokohama, and in November sails for San Francisco, where he spends the winter. May, 1888, finds him again in Tokio, and this year he escapes beri-beri. He stays all summer in Tokio, and all winter, and in June, 1889, he has a fourth attack of beri-beri. This time again he flies to Kochi, and recovers only in October. After recovery he reappears in Tokio in November, and spends there the winter and the spring. In May, 1890, he goes back to his native place before the beri-beri season begins, and escapes. He spends the following winter in Kobe. In 1891 he returns to Kochi, and spends the summer. He again is spared. (It must be observed here that in Kochi, his native place, there is but little charcoal used as compared with Tokio, and that the city, situated at the head of a seven-miles bay, is not surrounded by hills or fells, which might coop up the deleterious products of combustion: it was really from this carbonic poisoning that he was escaping during his sojourn at Kochi). The winter he spends in Tokio. In April, 1892, he goes to Osaka, having heard of the improved climatic conditions for beri-beri patients of that place, for the purpose of getting out of the range of the disease, but does not succeed. He is visited by it there and recovers in September, having been only one month sick this time. He spends the winter in Tokio, and in May comes to the United States.

He has neglected the only remedy which can have any real and lasting effect on his case; that is, in his own country, the mountain air. He is not cured, though his diet has been irreproachable, at least for years.

Dr. Toyama, who has charge of the beri-beri hospital at Usigomi, Tokio, has in his hospital, in the beri-beri season, from 100 to 200 patients. This establishment is situated on the highest ground of Tokio. A vegetable diet is imposed upon the patients; they get no milk, no meat, no fat fish. If they decline to remain in the hospital or do not improve, he orders them to the Hakoné Mountains, about eighty miles southwest of Tokio, or to the hot springs Mountain of Ikao at Nikko, eighty miles north of the capital.

An albuminous diet is not considered by this eminent physician as of signal importance for the cure of beri-beri: it is the altitude, even the moderate one of his own establishment, that does it. If one high place has no effect, he sends his patients to a still higher one. Does this suggest, in any human mind, the idea of rice and anœmia as the causes of a disease which disappears, almost at once, when the air is pure, rich in oxygen, comparatively free from carbonic emanations? If the cure takes place (and even in the Kakké season) where the degree of humidity is the same as, or greater than, in the beri-beri centres; and where the vegetable diet is compulsory, neither humidity nor anœmia resulting from a non-albuminous diet can be chief etiological factors of beri-beri, or, to express my opinion with complete frankness, can be factors at all.

One can hardly suppose that any merit in the cure of beri-beri patients can be attributed to the springs themselves around which the stricken herd gather. For why do not the same mild chalybeate and sulphur compounds (see Dr. Geert's analyses) operate in the same manner at sea-level. Hot bathing is also out of the question, it being in Japan a universal, almost passionate, habit. Consider also this fact: There are in Japan some excellent arsenic springs. It is well known that arsenic is the principal remedy for chloro-anœmia. Yet beri-beri patients find no benefit in them. There is, at any rate, no rush there, as would certainly be the case if beri-beri was an anœmia.

I have obtained, recently, some facts about the beri-beri situation in the island of Java, and I think I will append a few to this sketch: The Batavia beri-beri hospitals are situated at Buitenzorg, the old capital of Java. They are built on very high grounds; it takes a two hours ascending drive from the seaport to reach them. The patients are brought thither from the sea-level. The doctors in charge of these patients feed them rice and curry and eggs in different forms. The patients themselves, strange to say, take exception to a meat diet. The chief source of success, the doctors avow, is the climate.

In the whole of Java, the beri-beri outbreaks are at sea-level.

One thing is made evident by these facts: the beri-beri specialists, not only of Japan, but of Java, the cradle of the disease, have been taught by the most persuasive of all masters, long experience, that the cure of beri-beri has little or nothing to do with the diet, as they feed their patients even with vegetables. They seem to know by instinct that the disease must disappear as the red corpuscles are recreated by the ozone of the mountain air. It is not, as I view the matter, the condition of the red corpuscles, in itself, that causes the disease, nor does their rehabilitation in itself constitute the cure. But, as these red corpuscles reacquire the faculty of carrying oxygen, the carbonic toxine is eliminated, and with it the very root and soul of the disease, Dr. Takaki's rice and anœmia theory to the contrary notwithstanding. It is the elimination of the paralyzing element, carried by the blood, which, when thus recreated, the red corpuscles are

[2] For most of these facts, I am indebted to Trans. of the Asiatic Soc. of Japan.

able to bring about; it is this elimination, and nothing else, that constitutes the curative action.

I will now beg the reader to ponder over the two following facts, and see if he can reconcile them with Dr. Takaki's theory: 1st. The mountaineers of Japan, who have the reputation of being rice gluttons, eating, in fact, nothing else, are never afflicted with beri-beri. 2nd. There is, in the mountains of Japan, one beri-beri centre, and only one. What is more, this exceptional place is 800 metres above sea-level, it is called Shinano.[3] But see how strikingly, here, the exception confirms the rule. Shinano is again surrounded by higher hills, so that it is really a cup from which the carbonic gases cannot escape. The outbreaks of beri-beri in Shinano are explained by the latter circumstance, not by any extra rice-gluttony of the Shinanoans, or the excessive humidity of their climate.

[3] "Even the rule that the disease does not overstep certain quite low levels is shaken now; for the province of Shinano, walled in by mighty mountain chains, forms a plateau which, in many Kakke-ridden places, is raised 800 metres above the level of the sea. But, although these regions are not near the sea-level, they have yet a comparative depression; that is, they are low-lying plains, by the side of the circumjacent mountains, a circumstance of vast significance." BAELZ.

"Within the cities, also, the deep-lying parts show more cases of the disease than those of an elevated situation." BAELZ.

THE ORIGIN OF GOLD.

BY PHILIP LAKE, CAMBRIDGE, ENGLAND.

THE subject of the origin of gold, or of the manner in which that metal has reached its present positions, is one which has at all times excited considerable attention, and the number of theories put forward has been almost as great as the number of writers on the question.

It is easy to understand the presence of gold in alluvial deposits, for this has clearly been derived from preexisting rocks; but the difficulty lies in determining how the auriferous quartz-reefs and other rocks which we look upon as the home of the gold, became impregnated.

Sir Roderick Murchison, from his observations in the Ural Mountains, originally held that non-alluvial gold was only found in Paleozoic rocks, and principally in his Lower Silurian; but he believed that it was not introduced into these rocks until shortly before the Drift period. Subsequently he was led to modify these views to a certain extent, and to admit that Secondary and Tertiary strata when penetrated by igneous rocks or impregnated by mineral veins, might also contain gold.

More recent observations show that gold may be found in rocks of any age in metamorphic strata; but all the evidence seems to support Murchison's next contention, viz., that gold is of igneous origin.

There is probably no more instructive area to illustrate this than Southern India, where the distribution of gold has been carefully worked out by Mr. R. B. Foote, of the Geological Survey of India. Almost the whole of this part of India is made of crystalline and metamorphic rocks; and in it there are a large number of gold fields, more or less rich. A closer examination of the country shows that we have here a large mass of gneissic and granitoid rock which is crossed by a number of bands of schist, lava flows, hæmatite beds and conglomerates. Mr. Foote has shown that these bands belong to a system which is distinct from, and newer than, the gneiss, and to this system he has given the name of Dharwar. He has shown also that all the gold fields of Southern India, with the possible exception of the Wynaad, lie within these Dharwar bands.

As usual, the gold is found principally in quartz-reefs; and it is a remarkable fact that though quartz-reefs are by no means uncommon in the gneiss, as well as in the Dharwar beds, yet those in the gneiss are never auriferous. It is clear therefore that the gold cannot have been introduced into the reefs from below, for in that case there would be no difference in that respect between the reefs in the gneiss and the reefs in the Dharwar.

Only one other possible conclusion remains, viz., that the gold originally lay in the Dharwar rocks themselves, and that it has since, by some process of segregation, been gathered together in the quartz-reefs.

It has already been stated that lava-flows occur among the Dharwar rocks; and my own observations have led me to believe that many of the schists also are lava-flows. In fact a very large part, if not the greater part, of the system appears to be of volcanic origin.

It may be concluded therefore that the gold which we now find in the auriferous reefs of Southern India was derived from the rocks of the Dharwar system; and that it was originally brought up from the depths of the earth by the lava-flows which form so large a part of that system.

ON THE EXTREMES OF HEAT AND COLD UNDER WHICH THE LIFE OF SPECIES IS POSSIBLE.

BY HENRY DE VARIGNY, SC. D., MUSUEM OF NATURAL HISTORY, PARIS, FRANCE.

MARQUIS DE NADAILLAC contributed some months ago (January 27, 1893, page 49) to this paper an interesting note concerning the extremes of heat and cold endured by man, on the extremes of external temperature which man has been able to resist. The topic I wish to call attention to is entirely different. We all know that man, for instance, when resisting the extremes of heat and cold, hardly alters at all his internal temperature, and that when for some reason or other the latter decreases or increases, life is in great peril. To show the extremes of heat and cold man can endure is merely to illustrate the means he has at his disposal to fight heat and cold and to maintain his own internal temperature, and as these means are numerous and powerful, we may well feel assured that man may resist very extreme conditions by intelligent use of the offensive or defensive weapons he is provided with. The matter I wish to call attention to is the very reverse, in one sense, of the facts quoted by Marquis de Nadaillac. I wish to show which are the extremes of heat or cold which individuals may really undergo permanently, without damage to themselves and posterity. To answer the question, we need to consider organisms which have no proper heat to speak of, but assume the temperature of their environment; we want what generally goes by the name of *cold-blooded*, or *heterothermal* organisms, and we must have them aquatic, not terrestrial, because we very well know that terrestrial cold-blooded animals do not necessarily have the same temperature as the air which surrounds them; nor do plants. Air is a bad conductor of heat, and in air evaporation and transpiration prevent the temperature from going very high. So we want organisms living in water, because in this case, as they hardly produce any heat, they must necessarily have the temperature of the water they live in, moreover we want our organisms to be able to withstand heat or cold, not only individually, but specifically: they must resist as individuals and as members of a species, they must be able to proceed to reproduction. In fact, what we want is the permanent extreme degree of water (in heat and cold) under which organisms are able to live, and to give off posterity.

As far as I can jadge at present, these extreme degrees are, in Centigrade scale, minus 2° and plus 74°.

Arctic explorations have shown that even within the

coldest of northern regions life is never totally absent, and may be found when carefully searched for. But, it must be conceded, life becomes "living," so to say, only during a very short period, a rapid summer, during which the temperature rises above zero. The study of marine cold-blooded organisms, in the northern climes, furnishes, I think, the extreme limit of cold under which organisms can live and reproduce themselves. Fr. Kjellmann, during his wintering in Mosselbay (Spitzbergen) some twenty years ago, observed a number of algæ at the coldest period of the year, and was satisfied, by direct observation, that they did most decidedly give issue to the sexual elements, and that the process of reproduction was in full activity while the temperature of the water was permanently below zero, between -1° and -3° (salt water having a lower freezing point than fresh, about 3°). I do not know of instances of organisms thriving individually and specifically at lower temperatures, of organisms doing the same, while their internal temperature cannot be above that of the environment. Lichens must certainly be considered as living at much lower temperatures, since they perform the breathing function at -10°, -20° and at much lower aerial temperatures, but do they reproduce themselves under such conditions? Experiments are wanting, and till they have been performed, we may consider that the *lowest internal temparatures* at which organisms may *thrive and reproduce*, is -2° or -3°, and that some algæ do live under these conditions in the northern seas amidst the blocks of ice (Kjellmann: *Vegetation hivernale du Algues a Mosselbay, Spitzberg, apres les observations faites pendant l'expedition polaire suedoise en* 1872-1873: *Comptes Rendus de l'Academie des Sciences,* 1875).

As to extreme heat, I find no instance more satisfactory than that of Van Tieghem. In a paper, *Sur des bacterianes vivant a la temperature de 74° Centigrades* (published in the *Bulletin de la Societe Botanique de France,* 1881, Vol. 28), he has given the results of his experiments on certain bacteria, and has found that one species is able to thrive and to reproduce itself at 74°, while at 77° it dies. Many other micro-organisms can bear for some time 60° or 70° C., but I know of no other able to live permanently at 74° and to give posterity under such conditions. No doubt a large number of observers, of whom I have given some names, with the results they have obtained, in a paper: *Les temperatures extremes compatibles avec la vie,* (*Revue Scientifique,* 27 May, 1893), have given instances of plants and animals living in hot springs, and, if some were to be believed, animals and plants would have been found in boiling water. I do not say the thing is impossible, but great care must be taken when ascertaining the temperature of thermal waters. Hoffe Seyler has shown that under the uppermost layer of water, which may be very warm, colder layers are to be found, and animals may seem to live in heated water, when in fact they live in normal conditions. Unless special care is taken to observe the temperature at the very level where living organisms are found, we can take no serious account of the numerous and startling observations made by a number of travellers, and abstracted by Goeffert, formerly, and recently by H. Weed (*9th Ann. Rep. of U. S. Geol. Survey by Powell,* p. 619). There is no reason to suppose that no organisms can live and reproduce themselves at an internal temperature of more than 74°. Such organisms do doubtless exist, but we cannot feel assured of the fact yet. Persons who investigate thermal springs should be very careful in their measurements; correct observations can be of great use for the present question, although, in point of fact, I much prefer a good experiment, such as that of Van Tieghem's. But nothing prevents the completion of the observation by experiment.

BOOK-REVIEWS.

Abnormal Man: Being Essays on Education and Crime and Related Subjects. By ARTHUR MACDONALD. Washington: Government.

THIS is a goodly pamphlet of more than four hundred pages issued by the Bureau of Education, of which the author is an officer. It is of a somewhat desultory character, consisting mainly, as the author says in his preface, "of essays and of digests of foreign literature which have already appeared in different periodicals." These various articles, however, have been changed, more or less, and much new matter has been added. The object of the book is to inquire into the causes of crime with a view to their removal, and especially to consider the influence of education in repressing crime. It opens with a brief notice of the various classes of abnormal men, whom the author divides into four classes: the dependent class, including the inmates of almshouses, hospitals, orphan asylums, etc.; the delinquent class, or criminals; the defective class, such as the insane, imbecile, deaf and dumb and others; and finally, men of genius or great talent. The ranking of men of genius with the other classes mentioned is itself a rather abnormal proceeding, and the chapter in which the author endeavors to show that genius is nearly allied to insanity is likely to meet with little favor. His remarks on that subject, however, are aside from the main purpose of the book, which is to treat of the criminal class and the methods of eliminating or repressing it.

At the outset Mr. MacDonald raises the question whether and in what way the elementary education that has now become so general throughout the civilized world affects the increase or decrease of crime; and after presenting many tables of statistics on the subject, comes to the conclusion, which the reader is likely to share, that "the exact relation between education and crime is unknown." He remarks, however, that "it would be difficult to find a criminal who in a single instance could attribute the cause of his crime to education;" and adds that "perhaps as good a test as any is for one to ask himself if the teaching of ordinary branches in his school days gave rise to immoral or criminal desires." But if school education does not increase crime, there is not much evidence that it tends to diminish crime; and thus we are brought to the subject of moral education as distinguished from the intellectual sort, which is the chief product of the schools. Mr. MacDonald justly remarks that "while the moral and intellectual sides of education necessarily exist together, yet society is most solicitous about the former; for an individual may be a good citizen with little instruction if he has sound morality, but the reverse is not true." This, however, immediately raises the perplexing question, which is as old as Socrates, and which moralists of all ages have tried to answer, whether virtue can be taught, and, if so, by what means; but though our author realizes the importance of the problem, we cannot see that he contributes anything new to the solution of it.

The relation of education to crime, however, is only one of the topics discussed in this book, which deals with the whole subject of criminology with special attention to the question of preventing crime. In pursuing this theme the author says little directly about remedies, but confines himself mainly to the study of causes, on the ground that "all the conditions, occasions and causes of crime must be investigated first, if the treatment is to be a rational one." After pointing out the special topics for inquiry in criminology, he proceeds to set forth the views that have been advanced by leading writers on the subject in recent years, with special reference to the theories of the Italian school, which inclines to regard crime as a mental

disease. Mr. MacDonald's own views are expressed with caution, and in many cases he confines himself to expounding the ideas of the author he is dealing with, without offering any opinion of his own. The question of alcoholism in its relation to crime is treated at considerable length, and the views of many different writers presented; but, as is usually the case in discussions of that subject, the variety of opinions prevailing and the lack of sufficient information about the actual physical effects of alcohol result in leaving the question unsettled.

Mr. MacDonald's book contains much that will be useful both to those who are beginning the study of criminology and to the original investigator. To the former it will suggest the most important topics for investigation and the proper methods of work, while to the latter it will serve as a guide to the literature of the subject in all its departments. In this last-named respect the book is especially strong, since it gives not only a great many digests of recent works, but also an extended bibliography of the whole subject, filling more than two hundred pages. On the whole, though we do not agree with all the author's views, we have found his book on many points both interesting and suggestive.

LETTERS TO THE EDITOR.

***Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Animal Vocabularies.

Certainly one who believes in evolution cannot deny the existence of a language, of some sort, which enables the lower animals to communicate in a more or less intelligent degree.

Even my five-year-old little girl feels assured of the fact that animals can talk, "but not in our words." Only yesterday I sent her to the barn with an armful of fresh corn husks for our pony. She came running back with beaming countenance, exclaiming: "Daisy was so glad, she wanted to kiss me."

Several years ago I took great interest in some fine Brahma chickens we had raised from fluffy little chicks. There was one fine old grandmother hen which we bought to start with. She came recommended as a "good mother." And a good mother she proved to be, but she had her way of training a family. She went at it in earnest. She clucked and scratched and pointed out the best things to eat. She was fully impressed with the fact that she had a duty to perform, and she had the courage to devote herself entirely to this duty. But she always insisted upon early independence. She did not approve of chicks clinging to her and depending upon her when they were able to "scratch" for themselves, and hence she made it a rule to "wean" them early. She always gave them a parting lecture. She looked very wise and solemn, and "ca-cawed" in a peculiar tone, while the chicks stood about her in a sort of dazed, sorrowful way, wondering, no doubt, what would become of them. One "talk" ended the matter. She went off to roost alone, and the deserted chicks huddled together, "vaguely thinking" what a cold world.

Another interesting characteristic about this old grandmother hen was her solicitude for young hens who were just beginning to experience the first inclinations to sit. She would stand before their nests, and "talk" in the most earnest, subdued tones; her vocabulary must have been quite extensive, for she could continue without any hesitation for such a long time. It always seemed to me that she was relating her own experience and giving advice to the young and inexperienced of her kind. Certainly the young hens appeared to listen with all the respect possible—they no doubt "thought" that she magnified the cares and responsibilities; at least she never dissuaded a young hen from her resolution to *sit*. I agree with the writer in the last issue of *Science* (No. 549), who says "there is no need of going beyond the barn yard to hear a definite animal vocabulary of a considerable number of words."

If our language is the result of evolution, it has come up through lower forms, and it is only legitimate to credit animals with a varying degree of power of communicability. Mrs. W. A. Kellerman.

The Circulation in Fresh-Water Mussels.

In order to demonstrate the course of the circulation in a fresh-water mussel the student is commonly directed to make six injections: from the ventricle forward into the systemic arteries; backward through the auricles into the efferent branchial vessels; from the vena cava forward into the organ of Bojanus, and backward into the system; and into one of the branchial sinuses forward into the gills and backward into the organ of Bojanus.

I have, however, sometimes succeeded in demonstrating several of these connections by a single injection as follows: Cut away a small portion only of the outer lamina of the outer gill, make a little opening into the branchial sinus and with a very slow, steady pressure inject into it. The course of the injection may then be easily watched as it proceeds down the inner lamina of the gill, and after a little time begins to ascend in the outer lamina. Presently it will begin to escape at the cut ends of the efferent branchial vessels; enough of these are, however, left intact, so that most of the fluid passes on up to the auricle, thence into the ventricle, and it may be followed as it sets out from the heart towards the front and rear of the body on its systemic journey. At the same time, of course, the injection will flow from the starting point back into the efferent vessels of the organ of Bojanus.

I have not succeeded in continuing the pressure long enough or steadily enough to make the fluid pass on into the vena cava; the small systematic vessels seem to offer so much resistance that the injection is pretty sure to make a break somewhere before it finally succeeds in making its way through them; and in the same way the renal vessels fail to transmit it backwards into the vena cava. It is very likely that a steadier hand than mine might succeed better, or that an injection controlled by the force of gravity might be made to demonstrate the complete and orderly circuit of the blood around to the starting point; but even the injection of two-thirds of the entire circuit and the gradual progress of the fluid from point to point is instructive. Goodwin D. Swezey.

Doane College, Crete, Nebr.

Protective Mimicry of a Moth.

A correspondent of "*Science*," August 4, notes a case of protective mimicry of a moth. From the brief description given, the insect may be the Red Humped Apple-tree Caterpillar Moth, *Oedemasia concinna* which has just been reared from larvæ, at the University of Kansas, where work is being done in an economic and biologic collection of insects. About a dozen caterpillars were received from Delphos, Kansas, July 19, and after preserving two or three in alcohol, the remainder were put in breeding cages with apple leaves for food. By July 13, all had pupated, some going into ground at surface, while the majority made thin cocoons among the twigs and leaves in such manner as to be completely enveloped and hidden. Adults emerged by August 14, and then it was noticed how easily

they could be mistaken, while clinging to the limbs of trees, for short stubs of broken branches, and thus cheat their enemies out of a meal.

Taking this as the same species as described and figured in the article, it may be noticed that the distribution is wide, Ohio to Kansas, though it may be expected wherever apples are grown. From the adults, several lots of eggs were found on underside of leaves, and their development will be watched. E. S. TUCKER.

Lawrence, Kansas, Aug. 16.

EXPLOSIVE GAS IN LOCOMOTIVE ENGINES.

IN the article on p. 79 of *Science*, Aug. 11, 1893, concerning "Explosive Gas in Hot Water Apparatus," are some very pertinent questions to which I would like to add several in regard to high-pressure engines.

Assuming the facts stated as true, as they probably are, in the case of heating furnaces in houses, may they not be true also in, for instance, a locomotive engine under certain circumstances?

May not the hydrogen in a locomotive become mixed with common air?

May not this mixture be exploded under certain circumstances likely to occur in locomotives?

May not this be the real explanation of those sudden and terrific explosions that occasionally occur, where no apparent cause can be assigned? M. W. V.

Ft. Edward, N. Y., Aug. 16.

COYOTE OR BEAR?

COYOTE or bear? "that is the question" which has apparently agitated Dr. Franz Heger, Curator of the Ethnographical Museum at Vienna, ever since Mrs. Zelia Nuttall, Special Assistant in Mexican Archæology of the Peabody Museum, Cambridge, Mass., described and figured an ancient Mexican shield inlaid with feather-work and gold and bearing an animal device of a blue "monster" on a red field. (Internationales Archiv für Ethnographie, Vol. V., Part 1, 1892).[1]

This shield Mrs. Zelia Nuttall found preserved at Castle Ambras, in Tyrol, and, recognizing its unique character, obtained permission from the Imperial Oberhofmeisteramt at Vienna to have it sketched and photographed. It proved to be an ancient Mexican feather-work shield, with an authentic history, like the head-dress of the time of Montezuma, still exhibited at Vienna, "unfortunately always upside down." This was restored by Dr. Ferdinand von Hochstetter and described by him as a standard or banner.[2] Both head-dress[2] and shield were sent by Cortez to Charles V., and subsequently formed part of the historical collection of armor formed by his nephew, the Archduke Ferdinand of Tyrol, and were duly recorded in the Inventories of that famous collection. Strangely enough, the shield was supposed to be lost, and Professor Hochstetter lamented "its total disappearance." All the while it was lying *perdu*, in a case labelled "Transatlantic and Oriental Curiosities," at Castle Ambras in Tyrol, until its importance was recognized by Mrs. Nuttall on a chance visit to the Museum Ambras. Soon after Mrs. Nuttall announced the continued preservation and whereabouts of this valuable Ancient Mexican relic to the Anthropological Society of Berlin, and the shield was consequently removed to Vienna. Some other Ancient Mexican objects were also transferred there at the same time, and these Dr. Franz Heger has described in a memoir published in the Annals of the Imperial Natural History Museum of Vienna, 1892.[3]

It is not altogether surprising that the Austrian curators should have felt a little sore that the real history of so valuable a relic should have been forgotten, although the specimen was duly taken care of, and that its whereabouts and unique value should have been made known by a foreign visitor and Mexicaniste scholar. But that is no reason why Mrs. Zelia Nuttall's critical and searching investigations on "ancient Mexican shields" in general, and the Ambras shield in particular, should be misrepresented and misquoted. Any one reading Mrs. Nuttall's original memoir, and Dr. Heger's more recent article, cannot help seeing such to be the case. For instance, Dr. Heger curtly states, "According to Z. Nuttall the mon-

1. See "Ancient Mexican Heraldry," by Agnes Crane. Science, Vol. XX., No. 503, Sept., 1892.
2. "Standard or Head-dress," by Zelia Nuttall, Peabody Museum Papers. Vol. I., No. 1, 1888.
3. Altmexikanische Reliquien aus dem Schlosse Ambras in Tirol.

ster on the shield represents the fabulous Ahuizoltl, or water animal," whereas, while duly considering the possibilities of such identification, Mrs. Zelia Nuttall stated, in conclusion, "that she was prevented from upholding it," and drew attention to the resemblance between the outlines of the Ambras "monster" and those of the coyote or prairie wolf, as depicted in the Codex Mendoza to express ikonomatically the name of the Pueblo *Coyohuacan*—place of wolves. Dr. Edward Seler subsequently endorsed Mrs. Nuttall's identification of the Ambras monster as a coyote or prairie wolf.

Dr. Heger, however, declines to recognize the device as representing a wolf, and declares it to be a bear from "its fangs, claws and shaggy coat,"—characteristics, by the way, also common to the wolf. He admits that "the tail is rather long for a bear," but adduces, in support of his hypothesis, the fact that bushy tails are possessed by the smaller species of bears, and proceeds to evolve from his inner consciousness a Mexican species of small, long-tailed bear, unknown alike to ancient Mexican pictographers and more prosaic but exact modern zoölogists. Such authorities as Wallace[4] and W. H. Flower[5] state that only one species of bear, *Ursus ornatus*, is known to occur in the Neotropical region, which includes the American continent from the northern limits of Mexico to Patagonia, and that species is the spectacled bear, restricted to the Chilian sub-region.

Is it possible that Dr. Heger confused the *true bears* (Ursidæ) with the raccoons (Procyonidæ) familiarly known in Germany as "Waschbären," from their singular habit of washing their food. These, however, are not bears but small *bear-like* animals with long tails, commonly annulated. These raccoons do occur in Mexico, but they are characterized by "turn up" noses, which give them a mild and inquisitive appearance, differing widely from the wolverine aspect of the Ambras "monster," which looks as much like a wolf rampant with protruded claws as heraldic designs with that intent in general. The feet of the coyote or prairie wolf are more correctly indicated in the pictograph of the coyote from the Mendoza codex. The bears are flat-footed and cannot retract their claws, which form the only ursine feature of the Ambras monster.

Dr. Heger's fallacies, misquotations and self-contradictions are amusingly exposed by Mrs. Zelia Nuttall, in the current number of the *Internationales Archiv fur Ethnographie*, Part 6, 1893. To use a familiar metaphor, it will be seen that the lady has left neither Dr. Heger nor his hypothetical, long, bushy-tailed, small *Mexican bear* a leg to stand upon. *Fac-similes* of both the Ambras shield and the feather head-dress of the time of Montezuma are exhibited in the Ethnological Department of the Chicago Exposition. We believe Mrs. Nuttall is about to enter on the official duties connected with her appointment as "Judge of ethnological exhibits in the Women's Department," to which she has been recently nominated.

AGNES CRANE.

Brighton, Eng.

4. "Geographical Distribution of Animals," Vol. II., p. 201.
5. "Mammals Living and Extinct," p. 565.

EXCHANGES.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For exchange—Complete set of serial slides of embryo chick, 12 to 72 hours old; of rabbit lom long, and various stages of development of tadpole, mouse and rat. Will sell or exchange for botanical slides, or books or photographic apparatus. D. T. MacDougal, University of Minnesota, Minneapolis, Minn.

For sale or exchange.—A fine collection of Lepidoptera, native and exotic. For particulars address Addison Ellsworth, Binghamton, N. Y., care Republican.

For sale or exchange for works on entomostraca, Wolle's "Desmids of the U.S.," Hentz "Spiders of the U. S.," The Amer. Entomologist & Botanist, Vol. 2, The Amer. Entomologist, Vol. 1, Harris's "Insects Injurious to Vegetation," colored plates, copy formerly owned by Townend Glover. C. Dwight Marsh, Ripon, Wis.

"The Conchologist: a Journal of Malacology," Vols. 1 and 2, with wood cuts and plates, value 12] - will exchange for any works or pamphlets on American Slugs or Anatonry of American Fishes. W. E. Collinge, Mason College, Birmingham, England.

I wish to exchange a New Model Hall Typewriter, price $30, for a Daylight Kodak, 4x5 preferred. George A. Coleman, Dep't. Agric., Div. of Ornithology, Washington, D. C.

Exchange—The undersigned is desirous of obtaining correspondents interested in macro-lipidoptera, in Alaska, the far Western, Southwestern and Southern States. Will also exchange rare lepidoptera for entomological literature. Levi W. Mengel, Reading, Penn.

Wanted to exchange—Medical books, Obstetrical Transactions, London, Works of Sir J. Y. Simpson, Beck's Medical Jurisprudence. Handbook for the Physiological Laboratory, by Burnton, Foster, Klein and Sanderson, Quain's Anatomy, and about fifty others. Catalogues given. Want Geological, Botanical and Microscopical books in exchange. Dr. A. M. Edwards, 11 Washington St., Newark, N. J.

Wants.

I WOULD be grateful to receive replies to any of the following questions.—Is copper found native in Mexico? Is it found native in Cuba? If so, in either or both cases can I purchase authentic specimens? Can any one furnish me with results of analyses of native Mexican or Cuban copper, also analyses of unalloyed copper reduced from the ore from Cuba or Mexico? Is it possible to procure aboriginal implements of copper from Cuba or Mexico? Answers to these questions will greatly aid the preparation of a report for a scientific institution. C. B. Moore, 1321 Locust St., Philadelphia, Pa.

A YOUNG woman who has been an assistant for a literary and scientific man desires a similar position. Is an experienced and accurate stenographer and typewriter, thoroughly educated, and sufficiently familiar with literary work to write, independent of dictation. Has some knowledge of the Spanish language. Will go to any part of the United States. Address, Box 147, Ravenna, Ohio,

A GRADUATE of an American Polytechnic institution and of a German University (Gottingen), seeks a position to teach chemistry in a college or similar institution. Five years' experience in teaching chemistry. Address Chemist, 757 Cary St. Brockton, Mass.

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WANTED.—Assistant in Nautical Almanac office, Navy Department. The Civil Service Commission will hold an examination on August 15 to fill a vacancy in the position of assistant (computer) in the Nautical Almanac office. The subjects will be letter-writing, penmanship, trigonometry, rudiments of analytical geometry and calculus, logarithms, theory and practice of computations, and astronomy. Each applicant must provide himself with a five-place logarithmic table. The examination will be held in Washington, and if applications are filed in season, arrangements may be made for examinations in the large cities. Blanks will be furnished upon application to the Commission at Washington.